

reserving all speculation for a time when these facts will have been accumulated in sufficient number to afford a sound basis for more general inductions. "The book is a body of directions for collectors" (Preface vi.). It is divided into three chapters, one "On the Alphabet," another containing "Hints and Explanations," and a third supplying a large number of forms or "Schedules" to be filled up by the collector. The chapter on the Alphabet aims at establishing some uniform system of spelling for all the native tongues, and puts forth a comprehensive scheme embodying many useful suggestions well deserving the attention of our "spelling reformers." These are summed up in a few fundamental rules, the chief of which are the exclusion of all characters and diacritical marks except those found in ordinary English printing offices, and the restriction of each sign to a single sound. The difficulty of adapting the Roman system to the Indian tongues will be understood when it is stated that "there are probably sounds in each which do not appear in the English or any other civilised tongue; and perhaps sounds in each which do not appear in any of the others, and further, that there are perhaps sounds in each of such a character, or made with so much uncertainty, that the ear is unable to clearly determine what these sounds are, even after many years of effort" (p. 2). Nevertheless the difficulty is manfully faced and largely overcome by the scheme here adopted, which is founded on one originally proposed by Prof. J. D. Whitney, and which is consequently at once scholarly, simple, and comprehensive. A few improvements might here and there be suggested, but on the whole there is little to complain of, except perhaps the use of the circumflex (\wedge), to mark both a long *a* sound, as in *all*, and a short *u* sound as in *but*. Some confusion is caused by an awkward misprint at p. 5, where this \wedge appears instead of the German \ddot{u} . It might also perhaps be better to indicate excessive vowel length by doubling the vowel as in Dutch, than by the clumsy addition of the sign $\ddot{}$. Thus *maan* rather than *ma + n*.

Chapter II. contains a number of well-digested and tersely-expressed remarks on dress, ornaments, dwellings, implements, food, colours, plants, animals, medicine, social organisation, kinship, government, and many other topics, which at first sight seem to have little connection with the subject of American philology. But the author has wisely endeavoured thus "to connect the study of language with the study of other branches of anthropology; for a language is best understood when the habits, customs, institutions, philosophy—the subject-matter of thought embodied in a language are best known. The student of language should be a student of the people who speak the language; and to this end the book has been prepared, with many hints and suggestions relating to other branches of anthropology" (Preface vi.). But besides these matters the chapter contains what will be welcomed as a boon by all linguists, a reprint of J. H. Trumbull's masterly paper "On the Best Method of Studying the North American Languages," originally published in the *Transactions of the American Philological Association*, 1869-70, but strangely neglected by many subsequent writers on the subject. No other treatise perhaps of equal length contains so clear and philosophic an account of the peculiar genius and morphology of

these polysynthetic tongues. A great deal of space is devoted to the question of kinship, the true basis of Indian tribal society, and this intricate subject is fully illustrated by a series of four "kinship charts" or genealogical diagrams, which the original investigator will find of the greatest service in collecting and arranging his materials. The general student will also find them extremely useful in comparing the American systems of family relationship with those prevalent especially amongst the Dravidians of the Deccan and the Australian aborigines. Too much importance has perhaps been attached to resemblances of this sort in tracing racial affinities; but their significance in the history of the evolution of human culture is undeniable. Connubial society develops into kinship society, or the clan, in which all the members are blood relations, whence the tribe and nation. It is remarkable that the connubial, or lowest form, still so prevalent in many parts of the eastern hemisphere, seems to have long disappeared, at least from the northern half of the New World, although some of its customs, especially those associated with kinship, still survive in the more advanced tribal state. This explains the barbaric wealth of family nomenclature with which the Indian languages are still encumbered. In the printed forms, or schedules, of which Chapter III. exclusively consists, the terms of relationship occupy about forty pages, and include hundreds of complicate affinities such as, "my father's elder brother's daughter's daughter's daughter's daughter," "my father's mother's brother's son's son's son's son," "my mother's father's brother's son's daughter's daughter's daughter," "my mother's mother's sister's daughter's son's daughter's daughter," "my mother's elder sister's daughter's daughter's daughter's husband." For these, and even more intricate degrees of parentage, many native tongues supply equivalents, which the collectors are accordingly required to discover and insert in the blank columns prepared for the purpose in the schedules. The arrangement of the other matter contained in these schedules seems to be somewhat needlessly involved. At least the advantages are scarcely so obvious as the inconvenience of breaking up the strictly lexical part into upwards of twenty sub-headings, instead of lumping the whole in one general vocabulary arranged alphabetically. Experience has abundantly shown how troublesome is the use of such minutely-classified lists of words even for the compiler. This remark does not of course apply to the lists of sentences (Schedules 26-9), which appear to have been carefully prepared, and are well calculated to bring out the structure and varied grammatical forms of the Indian languages.

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LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Hot Ice

THE letter of Mr. Perry (*NATURE*, vol. xxiii. p. 288) in answer to mine on the subject of Dr. Carnelley's experiment (p. 264) has remained a long time unanswered, partly because I

was led by the letter to suppose that Prof. Ayrton himself might have something further to say regarding his views as soon as he returned to England, but mainly because I did not see any point in it specially requiring an immediate reply. I find however that a considerable amount of cautious scepticism and suspense of judgment still prevail on the subject—a scepticism which Prof. Herschel's enthusiastic letter of a month ago (p. 383) has not gone far to remove, because, though there can be no doubt of his confirmation of the fact that ice in a hot vacuum is *infusible* and disappears slowly, there is nothing in his letter confirming the hypothesis that it is *hot*, which is the only point under discussion.

Now for my own part I fully and unreservedly accept this as a fact, not only on account of Dr. Carnelley's experimental evidence, but also because I imagine myself to perceive exactly why it occurs, and indeed that it might conceivably have been conjectured as probable beforehand.

My present communication therefore is merely to remove as far as possible any sense of mystification which Prof. Perry's letter may have tended to produce, and to indicate the ground of his error.

Professors Ayrton and Perry, with their stiff paper models, start, if I am not mistaken, on the assumption that the ordinary equations deduced from the two laws of thermodynamics will apply to the case: and this is exactly how I started myself. I considered that it was necessary to investigate the behaviour of a substance whose properties were defined, not by two independent variables, as is usual, but by three; the pressure, quantity of solid, and temperature, being all three arbitrary and independent of each other in the Carnelley experiment; and I extended Clausius's general equations to suit this case. But it was very soon evident that they did not apply at all, and for this reason, that the second law is only true for processes that are *reversible*, and the sublimation of hot ice is essentially an *irreversible* process. This is indeed the whole gist of the matter, and it is entirely due to this that the ice gets hot. Ordinary evaporation of a liquid below its boiling-point against a pressure less than its "vapour-tension" is an irreversible process, and accordingly the temperature is perfectly indefinite, and depends on the rate of supply of heat and on the rate of evaporation. So also with ice above the boiling-point, that is, ice subliming under a less pressure than the vapour-tension; its temperature depends simply on the rate of supply of heat and on the rate of evaporation. So far everything is perfectly simple and absolutely certain.

The only possible question that can arise is whether internal disintegration of the solid will not set in and prevent its rising above the boiling-point: whether in fact a solid cannot boil as a liquid does. I have given reasons for believing that in a solid formed *in vacuo*, or without air-bubbles, and constantly rising in temperature, this will not occur; and I deny that under these circumstances it is in a particularly unstable condition analogous to that of superheated water on the point of "boiling by bumping."

This however I fully admit is a point distinctly open to discussion, and I imagine that without an experiment one could not feel at all certain about it. But personally I feel that the evidence already given us by Dr. Carnelley, together with the theoretical probability indicated in my former letter (p. 264), is sufficient and conclusive.

It was no doubt somewhat staggering to learn (NATURE, vol. xxiii. p. 341) that Prof. McLeod, with his well-known experimental skill, should have hitherto failed to repeat the experiment, or to get the ice at all above zero;¹ but I take this as an instructive example of those rare cases where refined experimental appliances are obstructions rather than aids, for I believe the failure to be simply due to the fact that Prof. McLeod's vacuum was far too perfect, and the evaporation therefore so rapid that the ice did not have a fair chance of showing its willingness to rise in temperature; it could not in fact get even as high as 0° C. But if Prof. McLeod will discreetly spoil his vacuum until the pressure is only just below the vapour-tension corresponding to the temperature shown by his thermometer, I have no doubt that he will see the ice rise to any

¹ Since this was in type I have received, by the kindness of M. Boutlerow, a copy of a paper read by him before the St. Petersburg Academy of Sciences, in which he summarises the views which have appeared on the subject, relates his failure to repeat the experiment, and confesses himself a sceptic. It would not be doing justice to M. Boutlerow's carefully-wrought memoir to discuss it in a foot-note, but it is my impression that his failure is due to the same cause as that which I have ventured to suggest above as accounting for Prof. McLeod's, viz. too perfect apparatus and too great experimental skill.

temperature he likes, and he will find that when it is crossing zero it will be utterly regardless of the fact.

The same kind of statement applies to solid carbonic acid, on which I have made a few experiments with a view to raising its temperature. I squeezed it into the ice form in a hydraulic press (to diminish the evaporating surface), put a thermometer in it, and held it over a fire. The evaporation is so excessively rapid, however, that it remains apparently just as cold as before.

I have not time to follow it up just now, but the obvious thing is to put it under pressure, so as to diminish the rate of evaporation, and then heat it. Prof. McLeod informs me that the boiling-point of CO₂ continues below its melting-point (which is given by Frankland as -57° C.), until the pressure is four atmospheres; so that anything just under four atmospheres may be applied to this substance with impunity, and it will then be exactly in the most favourable condition for the Carnelley experiment; and I have not the slightest doubt that it can then be warmed, and if at the same time the pressure be judiciously and gradually increased, that it can be made as warm as one pleases until it has all disappeared.

Experiment with substances other than water however are likely to be more difficult, simply because few substances have such a large latent heat both in the liquid and gaseous condition, and therefore few substances will be anything like so permanent and outlive the evaporation so long.

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THE announcement made some time since by Dr. Carnelley that ice *in vacuo* could be raised to a temperature far above its ordinary melting-point, seemed so thoroughly in opposition to the experience derived from the great work of Regnault on the tensions of vapours; and as it called for a complete change of ideas in a field in which I am much interested, and as Dr. Carnelley asked others to repeat his experiments, I was induced to examine for myself the experiments on which so curious a statement was founded.

I used two different methods: the Torricellian vacuum and the Sprengel vacuum. As the experiment, as conducted by the Torricellian method, can easily be repeated by any one, and is much simpler in form than Dr. Carnelley's, I shall detail it. In the first place I wished to obtain a clear continuous piece of ice round the thermometer, as Dr. Carnelley's method gave flaky ice, which I found might lead to errors, owing to its discontinuity leaving the thermometer bare in parts. To obtain clear ice the following method was used:—Some distilled water was boiled in a test-tube A fitted with a two-holed stopper, with a thermometer through one hole dipping into the water; when all the air was expelled, a glass plug was pressed into the other hole against the issuing steam, and the whole allowed to cool, and then frozen in a freezing-mixture. A long-necked "German Florence flask" was then rinsed with distilled water and filled with mercury, and also placed in a freezing-mixture. The tube A was then gently warmed with the hand, and the plug of ice adhering to the thermometer withdrawn. The glass plug in the second hole in the stopper was then replaced by a marine barometer-tube of about forty inches in length, having been drawn out about four inches from the top to facilitate sealing. The plug of ice round the thermometer was then inserted into the neck of the flask full of mercury, and the stopper pressed home. This caused the mercury to rise in the barometer-tube, and the whole was then inverted as at B; and when the mercury had all run out, the fall tube was melted through at the constriction B, leaving a Torricellian vacuum above. The flask was now laid on its side in a freezing-mixture and well covered over with ice and salt as at C. After a few minutes, to allow the receiver to cool, heat was applied to the neck of the flask with a Bunsen lamp, and even with a blowpipe, till the glass softened, but the temperature of the thermometer did not rise until some part of it became denuded of ice, or until air had been admitted. The experiment was repeated again and again, but in no case while the vacuum was intact could the temperature of the ice be raised materially above that of the receiver. If the temperature of the receiver was -12°, then the ice was a little over -12°, say about -11°, but never more than two degrees above the receiver, although the glass almost in contact with the ice was at its softening point. This is exactly what we would expect from Regnault's experiments; the temperature of the receiver determines the vapour-tension, and therefore the "boiling point" of the ice. The ice was certainly never hot, and was not even